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On a New Method of Sphygmographic Observation ; with Remarks on the Present Aspect of Vascular Physiology. By HENRY HARTSHORNE, M. D.

Some months ago, while standing upon the platform of a scale at a life insurance office, I noticed that a hand, whose motion on a dial-plate indicated the weight, vibrated in accordance with my pulse. Close inspection showed, not only that this accordance was exact, but that a definite series of movements could be observed ; four distinct impulses of the index hand. The first one was in the direction of diminution of weight, the hand moving as far as it would if my weight were lessened by half a pound. The second was in the opposite direction, as if by an increase of a whole pound in weight. The third was to the extent of a whole pound in the direction of diminution of weight ; and the last again in that of increase, half a pound. A trial some time later, under the depression of a temporary indisposition, gave, with the same order of succession, shorter movements by about one-third or one-fourth. Dr. Edward Hartshorne has, with different persons, observed considerable variations ; and has suggested, and contemplates having prepared, a registering apparatus ; for which, when constructed, I would propose the name *ballograph*, as a measure of the cardiac and vascular impulse.

Leaving such anticipated experimentation for its own future record, my present purpose is to call attention to this method of observation, and to remark upon the physiological explanation of the facts.

Reflection and actual trial, by careful horizontal and vertical movements of the arms while on the scale, have made it obvious to me, that the variation of the hand upon the dial-plate must have been produced by *impulses* of the circulation, in the one instance *upward*, with a force equal to the amount of deviation, and in the other instance *downward*. Now what could account for these alternate impulses ? The *time* of the whole series corresponded, without perceptible interval, with the time from the commencement of a pulsation at the wrist to its complete cessation ; three-eighths of that time, as I estimated it in my own person, belonged to the first two movements ; five-eighths to the last two. I believe the first, *upward* impulse (half pound) to be due to the expulsion of the blood from the left ventricle upwards into the aorta to its arch. The second, *downward* (one pound) to the descending movement of the great mass of the blood in the aorta and its branches. The third, again *upward* (one pound) to the general rebound of the arterial walls. The fourth, *downward* (half pound) to the closure of the semilunar valves of the aorta, and the consequent recoil or return of the blood.

These movements correspond nearly in succession with those recorded by Marey's sphygmograph, except that the complexity of their series is more distinct than the *dicrotism* of the latter. The upward impulse of the blood from the left ventricle to the arch of the aorta is, moreover, unmarked by

the sphygmograph. This last feature may, it appears to me, have some diagnostic value in affections of the heart. The second (downward) movement of the ballograph agrees with the first ascending line of Marey's instrument. The third agrees with the depression from the top of the wave in the latter. The fourth finds parallel in the sphygmographic mark, only when the latter is dicrotic. As to the theory of sphygmographic dicrotism, Marey ascribes it to the check of the aortic wave at the fork of the iliac arteries. But Naumann explains it more reasonably by the closure of the semilunar valves of the aorta; establishing this view by the proof that the dicrotous wave lessens in height in direct proportion to the distance of the vessel from the heart.

The occasion of these brief observations may serve to introduce a few words of remark upon the present aspect of vaso-motor physiology. Confident as were, a few years ago, and, indeed, still are, the declarations of Virchow and some others upon this subject, the invention of new modes and appliances of experiment has rendered necessary its re-examination. And, notwithstanding the (at least partial) adherence given by Marey, the most ingenious of late investigators upon it, to current views, I am bold to express the conviction that the facts have inaugurated some modification of those views, which must advance still farther.

Of the facts thus alluded to, some are the following. Chauveau observed¹ that compression of an artery, sufficient to stop the blood-current, does not always arrest the pulsation of the vessel; which has, therefore, an *autonomous* character, not simply dependent on the impulse from the heart. Bernard found, with his "differential manometer," that the *pressure* in an artery is *lessened* after division of the sympathetic nerve on that side. This refutes the opinion that the sympathetic causes by its innervation a *constraining* or *limiting*, instead of an actively propulsive contraction of the arteries.

Jonathan Hutchinson,² giving an account of seven cases of injuries to nerves, records, as (next to loss of sensation) the chief result of nerve-section, diminution of temperature. T. Nunn³ also describes a case of injury of the ulnar nerve, in which, two months after, the extremity had a temperature ten degrees lower than the other. Some similar observations have been made by Drs. Mitchell, Morehouse, and Keen. Such facts do not, of course, disprove the well-known immediate increase (sometimes of considerable duration) in heat following section of the sympathetic, as in Bernard's familiar experiment. But a distinction ought to be drawn, though it is generally overlooked, between the effects of a paralytic distension of and accumulation of blood in a vessel producing increase of heat, and a paralytic loss of power in arteries, gradually causing retardation of circulation, with permanent reduction of temperature.

In regard to secretion, Pflüger and Wittich have shown that it is influenced by nerves through their *direct* action upon the glands; as well as, or as they suppose instead of through the vessels. It is stated on the authority of Marshall⁴ that, in the *normal reflex* stimulation of the salivary glands, as by vinegar on the tongue, an active vascular excitement occurs, under which even the *veins pulsate*. We may then consider the

¹ Onimus and Viry, Journ. de l'Anat. et Physiologie, Nos. 1 and 2, 1866.

² London Hospital Reports, iii., 1866.

³ Pathological Transactions, 1866.

⁴ Treatise on Physiology, 1867.

checking of salivary secretion by powerful *irritation* of the sympathetic as altogether *abnormal* (pathological) and exceptional.

My observations, already spoken of, appear to me to suggest at least doubt in reference to the *dictum* of E. H. Weber upon the *pulse*; "pulsus est non materies progrediens, sed forma materiei progrediens." Experiments by the French physiologists have shown, for the pulse wave-motion, a rapidity of thirty feet per second; for the particles of blood ten and a half inches per second. My estimate, based on the results of the ballo-graphic use of the scale as above mentioned, would be about twelve feet in a second for the blood-impulse on the average. Doubtless both movements, the wave and the mass motion, *differ* in different vessels and even at different times. Actual absence of mass movement in the blood is of course not supposed by Weber or others. All are familiar with the mode of proving its rapidity by the transmission of a chemical substance, shown by tests, through the whole round of the circulation.

A number of experiments which have attracted much attention, suggesting among other things the improbable doctrine of physiological or normal *inhibitory* function in certain nerves, have been made with currents of electricity. On the question of the true meaning of these some light has been lately thrown by Onimus,¹ who reports a series of experiments with *continued* and with *interrupted* currents upon animals. He asserts that *inter-mitting* currents, applied to the cervical ganglia of the sympathetic, caused spasmodic tetanic contraction of the muscular coat of the arteries, and diminution of the temperature. *Continued* currents, on the contrary, produced an exaggeration of the *proper contractile* action of the vessels and *augmentation* of temperature. These results are by Onimus reconciled with those obtained by Bernard and Brown-Séquard, and others, by recalling the fact that the latter have always made use of *interrupted* currents. He also cites Legros' proofs that neither intermittent currents nor paralysis of vaso-motor nerves will produce *erection* in any of the erectile tissues.

The Mémoire of Chas. Legros upon the Anatomy and Physiology of the Erectile Tissues, is published in the first number of the *Journal de l'Anatomie et de la Physiologie* for this year. He demonstrates the extreme thickness and power of the muscular tissue of the arteries supplying erectile tissues, and declares that "c'est bien l'élément contractile des artères qui agit pour produire l'érection." In gallinaceous birds, the blood ascends into the comb against gravitation; there the muscular coat of the arteries is of the greatest thickness.

I may add to these citations only a reference to some recent experiments of Lortet, of Lyons, upon the rate of movement both of the blood and of the pulse, with the hæmodromograph and sphygmoscope. He states that "the dicrotism of the *speed* of the blood-current corresponds exactly with the dicrotism of the *pulsation*." This contradicts Weber's dictum of the *form* movement. Also Lortet ascertained, as had been concluded before, that "even while the heart is *at rest* the blood is always in active movement, often with considerable speed."

I wish to end these desultory remarks with a brief appeal to a very simple course of reasoning, in reference to the question of the true action of the contractile tissue of arteries; upon which I am obliged, without intending presumption, to hold a view in opposition to generally prevailing opinions.

¹ Comptes Rendus des Séances de l'Académie des Sciences, 1867, t. lxxv. p. 250; and Journ. de l'Anat. et de la Physiologie, No. 1, 1868, p. 94.

Obviously, the forces that *can* act upon the blood-current in the arteries are three: 1. The heart's impulse; 2. The elasticity of the arterial walls; 3. The proper muscularity or muscular contractility of the arteries. We may leave out of this question, however important otherwise, the *vis a fronte*, or capillary and nutritive force. Of the three forces just named, the order of sequence *in time* is important. First must come or does come the impulse of the heart; the others, in so far as they act, must follow. No one denies the action or reaction upon distension of the elastic tissue of the artery. Then, as to the muscular element, several things are *conceivable*; that it takes no part at all; that it is always in action; that it is in fixed or "tonic" contraction; that it acts before the elastic coat or tissue does, or at the same moment with that, or after that tissue. Now which of these is probable—which is certain? Since the muscular contraction of the arteries occurs even after death, emptying them, we must reject at once the idea of its taking *no* part during the living circulation. It is not and cannot be *fixed* or *constant*, because in that case the touch of the finger would detect it, and, in the intervals of the pulse, the touch finds *no* resistance at all in any healthy artery. All suppositions, arguments, and facts, contravene the opinion that the muscular tissue of the arteries is brought into action (under the rhythmic movement of the circulation, or in consequence of the impulse of the heart), *sooner*, for each beat, than the elastic reaction. The great difficulty in accepting the simple view of Sir Charles Bell upon this subject, which difficulty in part has induced the ruling out of his opinion as "obsolete," has been the usual slowness of the unstriped, involuntary muscular tissue in its reaction upon stimulation. If the two tissues, muscular and elastic, acted and contracted at the *same* moment, both having been at rest before the heart's impulse, the result would be that the muscularity of an artery would not influence the rate of blood-movement one way or the other; except that, when the semilunar aortic valves have closed before the contraction is over, the effect must be to push the blood forward, with a force proportionate to the sum of the elastic tension and muscularity combined.

Of all the five possible suppositions named, however, the last is the most probable; viz., that, neither elastic nor muscular tissue having been in action at the moment when the heart's impulse begins, the elastic reaction comes first, and the muscular contraction afterwards. In which case it appears to my mind an inevitable conclusion, that the total effect of the arterial muscular contraction, *coming behind* or after the contractile wave-movement produced by elasticity, *must* accelerate or force onward the blood. I will not pause to argue this point farther in detail; merely repeating that the other alternatives are, essentially, that the muscular tissue in the arteries is totally null in effect, or that it is all the time maintaining the arteries in a certain fixed degree of rigidity and calibre, such as the most delicate sense of touch or sight, applied to the largest or smallest artery, gives no information of, but only contradicts. Yet the view now taught in the most approved works on physiology, and made the basis of very important conclusions in pathology and therapeutics, is, that the sole function of the muscularity of arteries is to limit the flow of blood through them; the artery exerting its power most actively when it allows the *smallest* amount of blood to pass. This doctrine, which has really in its favour nothing but the misconstruction of certain experiments on arteries with interrupted currents of electricity, producing a morbid and exceptional tonic or rigid contraction, may well be reviewed now in the light of the facts which have been

just alluded to, as well as of those which concern the general laws of muscular contraction, newly investigated by Marey especially with great ability and success.

Experiments showing the Occurrence of Vegetable Organisms in Human Blood. By JOSEPH G. RICHARDSON, M. D., Union Springs, Cayuga County, N. Y.

In the course of my examinations of nearly one thousand specimens of human blood during the past year, I have, in a large proportion of cases, met with the molecular substance denominated by Prof. Salisbury "*zymotosis translucens*." In severe cases of rheumatism and neuralgia I have found long strings of these transparent granules, and occasionally homogeneous filaments; in the blood of patients afflicted with some other diseases, and of individuals enjoying comparative health, these particles were single, or adherent in rows of two to five or more, such rows often showing a tendency to become branched. They are doubtless identical with the so-called "globulins" of Donné (*Cours de Microscopie*, p. 85, Paris, 1844), and the "molecular substance" of Griffith and Henfry (*Micrographic Dictionary*, 2d edition, p. 92, London, 1860). But, in addition to them, I was at first surprised to find, in a few instances, that the blood contained in almost every field numerous minute, rounded particles, much more distinct than those above mentioned; not, like them, fading rapidly from view; having an active rotary or erratic motion, and strongly resembling the primary stage of certain infusoria, as seen in solutions of decomposing animal matter. They appeared in cases where the pulse was feeble and intermittent, the blood anæmic, and the powers of life at a low ebb; and diminished in number under tonic treatment, especially the administration of tincture of chloride of iron. It occurred to me that by this property of independent movement we might be able to recognize the existence of independent organisms within the blood, and thus obtain a strong presumptive evidence in favour of Prof. Salisbury's novel theories concerning the vegetable origin of disease; and in order to test the correctness of this surmise the following investigation was undertaken:—

Expt. 1. A drop of blood drawn from my own arm was placed upon a slide, and a minute portion of water, which had been standing four days upon some fragments of beef, and which, examined a few minutes before, exhibited multitudes of vibriones, was mixed with it, and the whole covered with a thin glass. Upon adjusting it under the microscope, the vibriones were found to be moving, some rapidly, some slowly, and some only as borne by the currents among the blood-corpuscles, apparently unaffected by the change of world which they had undergone. By arranging a filament of thread from the reservoir upon the growing slide, so as to supply the loss of fluid by evaporation, I was able to watch their progress, at short intervals, for about nine hours, which was, as far as I could judge, uninterrupted towards the formation of vibratile filaments resembling the early stages of development in the so-called *Leptothrix Buccalis* found so abundantly in the tartar on the teeth. One particular filament, which was carefully watched, and of which drawings were made from time to time, whose movement, when first observed, was very active and constant, grew from a length of about $\frac{1}{3500}$ th of an inch to that of about the $\frac{1}{1000}$ th of an inch in eight hours, to that of about $\frac{1}{800}$ th of an inch in seventeen hours; and at the end of twenty-three hours, when the experiment was interrupted, had attained a length of about $\frac{1}{500}$ th of an inch. The process of development seemed to be accompanied with a disposition to bend sharply at intervals of perhaps $\frac{1}{2000}$ th of an inch, and shoot forth from the salient angle a branch equal in size to the parent trunk. As the organism increased in length, its movement diminished in rapidity, until towards the close of the experiment it nearly or quite ceased;